the condensation of vapour on a nucleus of ice ; and that it is impossible that the particles of ice can have been drawn together by electrical attraction—their conical shape, and the increase in their density towards their thicker sides clearly showing that the particles have aggregated from one direction, and with an increasing force as the size of the stone has increased.

The possibility of making artificial stones is thus considered :—If a stream of frozen fog were driven against any small object, then the frozen particles should accumulate on the object in a mass resembling a hailstone. Not seeing his way to obtain such a stream of frozen fog, the author thought it might be worth while to try the effect of blowing very finely powdered plaster of Paris. He therefore introduced a stream of this material into a jet of steam, issuing freely into the air (which he hoped would moisten the powdered plaster sufficiently to cause it to set firmly into whatever form it collected). The jet was directed against a splinter of wood.

In this way masses of plaster very closely resembling hailstones were obtained. They were all more or less conical, with their bases facing the jet. But as might be expected, the angles of the cones were all smaller than those of the hailstones. Two of these figures are shown in the sketches annexed :

The striæ were strongly marked, and exactly resembled those of the hailstone. The bases also were rounded. They were somewhat steeper than those of the hailstone ; but this was clearly due to the want of sufficient cohesive power on the part of the plaster. It was not sufficiently wet. Owing to this cause also it was not possible to preserve the lumps when they were formed, as the least shake caused them to tumble in pieces.

Similar masses were also obtained by blowing the vapour of naphthaline, but these were also very fragile. Whereupon it is remarked :—At ordinary temperatures the powdered naphthaline does not adhere like ice when pressed into a lump. No doubt at very low temperatures ice would behave in the same way, that is to say, the particles would not adhere from the force of impact. Hence it would seem probable that for hailstones to be formed, the temperature of the cloud must not be much below freezing-point.

That the effect of the temperature of the cloud exercises great influence on the character of the hailstones cannot be doubted. And if, as has been suggested by M. L. Dufour, the particles will sometimes remain fluid, even when the temperature is as low as o° F., it is clear that as they are swept up by a falling stone, they may freeze into homogeneous ice either in a laminated or crystalline form.

The author then proceeds to show that raindrops are probably formed in the same way as hailstones ; that although the raindrops have no structural peculiarities like the hailstones, the aggregation of the particles of water by the descent of the drop through the cloud is the only explanation which will account for them. He shows that, as Mr. Baxendell had previously pointed out, the amount of vapour which a cold drop could condense before it becomes as warm as the vapour would be inappreciable when compared with the size of the drop, and since, in order that there might be condensation, the air must be warmer than the drop, the drop could not part with its heat to the air. He also shows that during the time of descent of a large drop, the heat lost by radiation would not account for the condensation of sufficient vapour to make any appreciable difference in the size of the drop. Whereas if we suppose all the vapour which a body of saturated air at 60° F. would contain over and above what it would contain at 32° to be changed into a fog or cloud ; then if a particle, after commencing to descend, aggregated to itself all the water suspended in the volume of air through which it swept, the diameter of the drop after passing through 2,000 feet would be more than an eighth of an inch, and after passing through 4,000 feet a quarter of an inch, and so on. So that in passing through 8,000 feet of such cloud, it would acquire a diameter of half an inch.

The fact that raindrops never attain the size of large hailstones is explained as being due to the mobility in the case of large drops of the surface tension of the water, by which alone the drop retains its form, to withstand the disturbing force of the air rushing past ; when the drop reaches a certain size, therefore, it is blown in pieces like the water from a fountain.

The origin of drops and stones is then discussed—why some of the particles in a cloud should be larger than the others, as it is necessary for them to be in order that they may commence a more rapid descent. A cloud does not always rain ; and hence it would seem that in their normal condition the particles of a cloud are all of the same size and have no internal motion, and that the variation of size is due to some irregularity or disturbance in the cloud.

Such irregularity would result when a cloud is cooling by radiation from its upper surface. The particles on the top of the cloud being more exposed would radiate faster than those below them and hence they would condense more vapour and grow more rapidly in size. They would therefore descend and leave other particles to form the top of the cloud. In this way we should have in embryo a continuous succession of drops.

Eddies in the cloud also form another possible cause of the origin of drops and stones.

D'ALBERTIS'S EXPEDITION UP THE FLY RIVER, NEW GUINEA

RECENT letters from Sydney announce the successful results of Signor L. M. D'Albertis's expedition up the Fly River, and that he was shortly expected back in Sydney.

The following letter from him to Dr. George Bennett has been published in the Sydney newspapers of October 13 :—

"DEAR DOCTOR,—I have written a letter to the Committee, necessarily very brief, as I have but little time and a very scanty supply of paper. I am satisfied with the collection I have made, not for the number, but for the quality. I have four species of birds of paradise (*Paradisea*), the *P. raggiana*, the *P. apoda*, the twelve-wired bird of paradise (*Seleucides alba*), the king bird of paradise (*Cincinurus regius*), and the rifle bird (*Epimachus magnificus*). I got a perfect adult specimen of a cassowary, which I think is *Casuarius bicarunculatus* ; also the *Dasyptilus pecqueti*, a new genus of *Ptilotis*, and a splendid new species of *Gracula*, and several other small but very interesting birds. I have seen many birds which are not included in the avifauna of New Guinea, as the *Pelecanus conspicillatus*, the Jabiru (*Mycteria australis*) and the pygmy goose (*Nettapus pulchellus*). Among my fishes I have some fine and large species. Of reptiles I have very few except a water snake, which I hope will be something extraordinarily new. Among my insects I have some fine Coleoptera, but the season was not very favourable for them. I expect to have about five hundred species of dried plants and between twenty and thirty of living plants, collected far in the interior, many of which I did not get afterwards. I hope Mr. C. Moore will be satisfied, as I have some fine crotons and palms among them, also some ferns with variegated leaves, orchids, and several other plants with variegated or spotted foliage, &c., from the very centre of New Guinea. I hope Mr. Moore has sent to Somerset some Wardian cases, so that the plants may not be destroyed by the sea breezes during the passage to Sydney. I much regret that I cannot send you any specimens, but I have not a box to pack them

in ; the few boxes I have are filled with earth and living plants. My ethnological collections are very extensive indeed ; I have literally cleared all the houses, and I have the best collection of the stone implements of New Guinea ever seen, of every kind and description. I have also the ornaments used when dancing and when engaged in war, paddles for their canoes, &c., &c. I procured dresses of various patterns, some petticoats made of human hair, others of grass, both of the natural colour and dyed ; stone implements, finished and unfinished ; painted and carved skulls, stuffed human heads, arrows pointed with bone, artistically worked, and the cement used in fixing the points. I am very anxious to show you everything, and see your surprise at the beauty of my collection, as you can so well appreciate it. I hope my plan of the Fly River will be correct. I have noted mile by mile, and every day I landed I recorded the nature of the soil, &c. I hope the Government, Committee, and subscribers to the Expedition will be satisfied with the confidence they placed in me, and, more so, when I have time to publish my notes *in extenso*. I have investigated science in all its various branches, more especially anthropology. The presence of the great bird of paradise (*Paradisea apoda*, Linn.) in the centre of New Guinea, but at the same time in almost the same latitude as Aru Island, is of the greatest importance after what Lesson has asserted, and which has been denied by Wallace. I have got specimens in every stage of plumage, and of both sexes, and I have no doubt it is the *P. apoda*, and not the *P. papuana*. It is, nevertheless, much smaller than all the specimens I have seen in the British Museum, and in the collections of Mr. Beccari and Mr. Cockerell, and if with this distinction, when compared, any other difference may be perceptible, then it will probably prove to be a new species. For the present I believe it to be the *Paradisea apoda*. I have two beautiful male birds in full plumage, and also of the *P. raggiana*. I hope the Committee will be pleased with the short report I have sent, but at the same time must ask them to suspend their judgment of all that I have done until they receive a more extended and minuter account of the expedition from me." [Mr. D'Albertis concludes his letter with a few lines addressed to Mrs. Bennett, in which he says] : "I am in good health and spirits, and remember your kindness, for I bought bananas, when I was starving, with the red worsted ribbons adorned with pearl shells you gave me to traffic with the natives. I also enjoyed the large plum cake for a month, and finished it in the true centre of New Guinea, and wished I had another. The flag was the admiration of the natives of Moatta, and I bore in remembrance the ladies who presented it to me."

P. L. S.

OUR ASTRONOMICAL COLUMN

THE NEW STAR IN CYGNUS.—It is stated by Prof. Littrow (*Bulletin International*, December 12) that this star, which on December 1 appeared of about the same brightness as at the time of discovery on November 24, was of the fourth magnitude on December 2, and two days later had descended to the fifth.

Comparisons with neighbouring stars, using the magnitudes of the "Durchmusterung" on December 13, showed that the estimate we gave last week was somewhat too low, being doubtless influenced by the unfavourable conditions on the previous night ; the star was found to be 5·8—6·0, but as before, without trace of colour.

The following position for 1876·0 will be rather closer than the one given last week, R.A. 21h. 36m. 50·41s., N.P.D. 47° 43′ 21″·5, to which correspond annual precession in R.A. + 2·361s., in N.P.D. − 16″·27. We give M. Cornu's spectroscopic results in another column.

REMARKABLE STAR SPECTRUM.—D'Arrest, writing in November, 1873, refers to the spectrum of the star XX. 1396, of Weisse's second catalogue from Bessel's zone observations ; he says "sein Spectrum ist das *merkwürdigste unter einigen tausenden*, die ich bislang untersucht habe," and thinks the star may eventually prove to be variable. Has this object been examined by any British spectroscopist? It was observed by Bessel as an eighth magnitude, October 26, 1823, and his position reduced to 1877·0 is in R.A. 20h. 43m. 24·2s., N.P.D. 67° 27′ 36″.

THE MINOR PLANET, No. 169.—This last discovered member of the group of small planets which was detected at Paris on September 28, has been named "Zelia." M. Leverrier's *Bulletin* of December 12, contains ample materials for the determination of its orbit.

NEWCOMB'S CORRECTIONS TO HANSEN'S LUNAR TABLES.—Part III. of papers published by the United States Commission on the transit of Venus, just received, contains an important investigation, by Prof. Newcomb, of the corrections required by Hansen's lunar tables, for the purpose of rendering the lunar ephemeris available for accurate determination of the longitudes of stations not telegraphically connected with well-ascertained positions.

Remarking that determinations of longitudes from moon culminations have been found by experience to be subject to constant errors which there is difficulty in allowing for, Prof. Newcomb refers to its having been a part of the policy of the American Commission to depend rather upon occultations. An occultation of a star is a sudden phenomenon, and the time at which it occurs can be fixed by observation within a small fraction of a second ; wherefore, if the ephemeris of the moon is exact and her figure a perfect circle, the longitude could be determined from such observations with a similar degree of precision. The inequalities of the lunar contour form a source of error that it is impossible to avoid, but may be considered to be eliminated from the mean of a large number of observations, and the star's position admitting of being fixed by the meridian instruments with any required exactness, there remain only the errors of the lunar ephemeris to be diminished as far as practicable, and it is the object of Prof. Newcomb's paper to reduce these errors to a minimum.

The material principally relied upon is the series of meridian observations of the moon at Greenwich and Washington from 1862 to 1874, but in order to verify the most striking and unexpected result of the investigation, the comparison of Hansen's tables with the Greenwich observations during the twelve years 1847-1858 has also been utilised. The result alluded to is the irregularity in the moon's longitude represented by

$$1''{\cdot}50 \sin (56°{\cdot}8 + 13°{\cdot}124136t)$$

where t is reckoned in days from Greenwich mean noon of 1850, January 0. The period of this inequality is 27·4304 days.

Prof. Newcomb remarks that "it would perhaps be premature to introduce so purely empirical a term as this into lunar tables for permanent use," but in the particular case to which his researches apply, where it is requisite to obtain the corrections of the tables with all possible accuracy for a limited period only, he considers the evidence in favour of the existence of the inequality sufficiently strong to justify its introduction. He further observes that the only apparent cause for this term is "the attraction of some one of the planets."

Prof. Newcomb finds some support to a correction of the tabular longitude of node, as already suggested by Hansen in the *Darlegung* in connection with his discussion of ancient eclipses. The entire corrections to the moon's longitude given by his investigation are given at p. 37, supplemented by auxiliary tables for facilitating the calculation of the corrections required by the tables as published, the arguments in which extend from 1850 to 1890.